

graphical statics. They are well devised and very clearly put to the candidates. The wording is often such as to act persuasively on the examinees. That the questions are also up to date is indicated by the presence of some on aeroplanes and kites.

Answers are given by the editor in the case of questions of a mathematical or arithmetical nature. There is a misprint on p. 11 of the "Mathematical Papers," line 6 from the bottom. S. B.

A Short Hand-book of Oil Analysis. By Dr. A. H. Gill. Revised eighth edition. Pp. 209. (Philadelphia and London: J. B. Lippincott Co., 1918.) Price 10s. 6d. net.

THIS is a handy little book for a student of oil chemistry to commence his technical practice with. It is intended as an introduction to larger works such as that of Lewkowitsch, and deals with the chief animal and vegetable oils, petroleum products, and the various greases used as lubricants. It gives the essential information briefly but clearly, and includes a good number of references to original sources of information. The volume is written from the American point of view, and some of the apparatus mentioned is more familiar in American laboratories than in this country. Some of the books quoted, also, are not readily accessible here. The British reader, however, will have no difficulty with the greater part of the work, and he will find it a very useful guide. The first paragraph on p. 175 wants revision: it appears to have suffered in the press.

LETTERS TO THE EDITOR.

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The Perception of Sound.

THE recent publication of Sir Thomas Wrightson's valuable and extensive investigations on the functions of the various parts of the auditory mechanism has brought into prominence a fundamental divergence of opinion as to the place where analysis of the complex sound vibrations occurs and as to the mode of vibration of the basilar membrane. The view of Helmholtz may be said to be that most generally accepted at present. As is well known, this theory states that the basilar membrane responds by resonance in different parts to the component waves of the complex, and that each of these components gives rise to its own sensation on arriving at the brain. The analysis takes place in the cochlea. Sir Thomas Wrightson's theory, which has received the powerful support of Prof. Keith, states that the basilar membrane as a whole follows in its wave form that of the complex, that the form of this complete compound wave is transferred to the nerve-fibres, and that no analysis takes place until the brain is reached.

Careful consideration of the evidence brought forward in support of this view has aroused several difficulties in my mind which, I venture to think, require explanation.

In the first place, there are certain physiological facts which make it extremely difficult to accept any

sort of transmission of a complex wave form through a nerve-fibre. The work of Keith Lucas and his colleagues has shown that the process set going in a nerve-fibre has a definite time-course and magnitude, whatever be the way in which it is produced. If a sound-wave be enabled to stimulate a nerve-fibre by some appropriate receptor organ, the nerve process will be the same, however different the form of the wave. If this be true, it implies the necessity for a peripheral analysis, if there be any analysis at all. A similar difficulty arises in connection with the perception of notes of high pitch. If Sir Thomas Wrightson's theory is correct, the number of impulses passing along the nerve must be the same as the number of vibrations in the note, or possibly two or four times the number. The frog's nerve is incapable of responding to a second stimulus if it arrives less than $2/1000$ ths of a second after a previous one. It would, therefore, record all rates above 500 per second as identical. Doubtless this "refractory period" is shorter in the warm-blooded animal, but it is scarcely likely to be short enough to enable responses to 40,000 per second to be transmitted in their exact form.

It will be noticed by the physiological reader that Müller's law, which appears to have been first put forward by Sir Charles Bell, is involved here. It has been found that the sensation evoked from any nerve of special sense is identical, whatever the kind of stimulus applied. The peculiar quality of touch, taste, light, and so on, is due to the way the fibres end in the brain. Further than this in the way of explanation is at present impossible. But are we to suppose that the auditory nerve is the only exception to this law? What we should naturally expect would be that activity in one particular fibre, or, perhaps, set of fibres, in this nerve would be associated with the perception of one single definite tone, and that the form of the stimulus would be a matter of indifference. The theory of Helmholtz presents no obstacles in this respect.

In the second place, there is a physical question about which there seems some confusion. Many physiologists would be grateful to Lord Rayleigh if he would put us right here. There is no dispute as to the impression by the stapes on the liquid of the scala vestibuli of a series of impulses, corresponding in wave form with those air vibrations received by the membrana tympani. But, with the exception of the fact that the pressure is intensified, I am unable to see how these vibrations, when they arrive in the liquid, differ from those which would be present in the liquid if it were conducting sound in the ordinary way. It appears to be forgotten sometimes that liquids could not conduct sound unless they were both elastic and compressible. But the latter quality is, of course, extremely small, so that the amplitude of the vibrations is very minute. The actual movement in space of the column of liquid as a whole, contemplated by the Wrightson theory, is quite different, and, so far as I can see the inertia of the mass would make it impossible for the force available to effect such movements at the rate of several thousand per second.

There is another point involved here which I confess to an inability to understand. When the stapes pushes in the membrane of the fenestra ovalis, the movement of the liquid shows itself simultaneously by a protrusion of the fenestra rotunda. Now the basilar membrane forms part of a partition between the two columns of liquid in the scala vestibuli and the scala tympani. If these columns were not connected at the apex of the cochlea, it is clear that the pushing in of the stapes must cause a bulging of the

membrane into the scala tympani. It may be due to some oversight on my part, but I cannot see how there is a difference of pressure on the two sides when the column of liquid is a continuous one. This is a fundamental question in the Wrightson theory. It may further be pointed out that if the vibrations in the liquid to which the basilar membrane responds are the same as those of sound, there is naturally no difficulty with regard to hearing through the bone when the stapes has become fixed.

In the third place, objections made to the possibility of the basilar membrane acting as a series of resonators seem to me to have forgotten some facts pointed out by Helmholtz himself. The rate of vibration of a string is related, not only to its length, but also to its tension, and there is no reason why the range of the basilar membrane should not be increased by being more tightly stretched at its narrower end. So far from it being necessary to have a series of separate strings, Helmholtz demands only that a membrane of the shape of that of the basilar membrane should be more tightly stretched transversely than longitudinally, and in an appendix to the "Tonempfindungen" he gives a complete mathematical analysis of the vibration of such a membrane. I regret that this is beyond my mathematical capacity, but we may surely accept it. Prof. McKendrick has shown experimentally that resonance is possible in membranes immersed in water, as would be expected from theory.

On the other side, Held has stated that a single fibre of the cochlear nerve may supply a comparatively long stretch of the basilar membrane—undoubtedly a difficulty in the Helmholtz view. But the statement requires confirmation, and it must be a matter of great difficulty to decide it.

The explanation of the way in which the movements of the basilar membrane are altered in direction so as to stimulate the hair-cells is given very clearly by Sir Thomas Wrightson, and I have no objections to make to it. On the other hand, it is not clear why there are so many Corti arches and nerve-fibres present. One would have thought that a few would suffice to transmit the vibration if it occurs throughout the membrane at the same time and in the same form, no analysis taking place.

Sir Thomas Wrightson does not refer in his book to the interesting experiments of Yoshii, who found localised lesions in the organ of Corti as a result of prolonged exposure to a musical note.

Prof. Keith describes in great detail the minute structure of the organ of Corti, and states that various structures conform more to what is demanded by the Wrightson theory than to that of Helmholtz. I would demur somewhat to his view that every structure must have its function, although with deference to Prof. Keith's wide knowledge of the question.

It must be admitted that there are objections to be brought to both theories; but, on the whole, it seems to me that there are no vital ones to that of Helmholtz, whereas there are some to that of Sir Thomas Wrightson unless a satisfactory answer is forthcoming to those pointed out above. W. M. BAYLISS.

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Rainbow Brightness.

It would, I suppose, be very difficult to compute theoretically the brightness of any selected part of a rainbow in terms of the sun's actual brightness.

But it occurred to me that we might compute the relative brightnesses of two selected portions of the primary and the secondary bow, simultaneously presented and situated along a common radius. The luminosities would be due to the same sun, and to raindrops of practically the same size, so that any

differences would arise from the fact that for the primary bow there is one internal reflection, but for the secondary there are two, and from the fact that the angular constants differ. This difference in the angles will affect both the polarisation and the intensities of the light reaching the eye, and will also increase the breadth of the secondary bow to about 1.61 of that of the primary, thus further reducing the brightness of the secondary.

But by restricting attention to portions of the red in each bow, situated along a common radius, I think we may leave out of account the influence of the increase in breadth.

Let P , S denote the brightnesses of the two portions selected (along a common radius) from the red of the primary and of the secondary bow. Then, using Fresnel's formulæ, with $\mu=4/3$, I get

$$P = \frac{k}{22.40}, \quad S = \frac{k}{52.20},$$

where k is an unknown factor. Thus $P/S=2.33$ for the relative brightness of the red of the primary to that of the secondary, as viewed by the naked eye.

Now the light of both bows is very considerably polarised in the planes of reflection, which are radial and pass through the sun, the eye, and the element of arc observed. For the primary, 96.69 per cent. is polarised in, and only 3.31 per cent. polarised perpendicular to, the reflection plane. For the secondary, the corresponding quantities are 90.64 per cent. and 9.36 per cent.

Through a Nicol prism, placed first so as to transmit the "in" light, and next so as to transmit the "perpendicular" light, we get for the primary bow

$$\frac{P(\text{in})}{P(\text{perp.})} = 29.20.$$

Similarly, for the secondary bow:

$$\frac{S(\text{in})}{S(\text{perp.})} = 9.69.$$

Thus for the two positions of the Nicol the primary bow is reduced about twenty-nine times, and the secondary nearly ten times.

I have often experimented on the primary bow, and the element of arc practically disappears for the second position of the Nicol. I hope to experiment similarly on an element of arc of the secondary bow, which, though losing a smaller fraction of its light, is originally fainter than the primary, and so may also be expected to disappear, though for the "perpendicular" azimuth of the Nicol the remnant left of the secondary bow is a trifle (1.21 times) brighter than that of the primary.

Finally, we can compare the brightnesses of the bows, first with the Nicol in, and next with the Nicol perpendicular to, the polarisation plane:

$$\frac{P(\text{in})}{S(\text{in})} = 2.49,$$

so that the relative brightness with the Nicol slightly exceeds 2.33, that to the naked eye, when the Nicol transmits its maximum of each.

$$\text{Next, } \frac{P(\text{perp.})}{S(\text{perp.})} = 0.83,$$

so that the primary bow, as already stated, is slightly fainter than the secondary, when each is reduced by the Nicol to its minimum value. It would be of interest to test these cases by observation.

For the red light I gave 2.33 as the ratio of P to S for the naked eye. But to me the primary bow, viewed generally, appears brighter than this in comparison with the secondary. It is very difficult to make any true comparison on account of the varied colours. If we may assume that the secondary bow,